What do we know about innovation? Lessons from the TEARI project

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(*) This report is based on the work undertaken within the TEARI project, the main results of which will be published as "The Oxford Handbook of Innovation" (OUP 2004), edited by Jan Fagerberg, David C. Mowery and Richard R, Nelson. However, the views expressed here are those of the author, and do not implicate the other participants in the project or its financial sponsors; the European Commission and the Norwegian Research Council. The author wishes to thank Ovar Andreas Johansson and Fulvio Castellacci for helpful assistance and Nikos Kastrinos, David Mowery, Bengt Åke Lundvall and Bart Verspagen for comments on an earlier draft.

Introduction.

Innovation, including its causes and consequences, has been a central topic for scholars, analysts and policy makers for some time. The last two decades have seen the formation of many new research centres devoted to this, particularly in Europe and Asia, often with a bent towards cross-disciplinarity, and several new journals and associations have been founded. The number of publications focusing on innovation has soared (Fagerberg 2004), and several research initiatives, not the least within the European Union's framework programmes, have made this a central priority. Taking these trends as its point of departure the TEARI project (see box 1) has attempted to create an overview and synthesis of the present scholarly literature on innovation, and discuss the implications for the future research agenda. This paper presents, in a condensed form, some of the main conclusions from this work, and raises the question of to what extent these insights concur with the priorities of the European Union's framework programmes.

Box 1. The TEARI project

The TEARI project – Towards a European Research Area of Research and Innovation – was initated by Professor Jan Fagerberg at the Centre of Technology, Innovation and Culture at the University of Oslo, and supported financially by the European Commision and the Norwegian Research Council. The purpose of the project, which lasted two years (2002-2004), was to create an overview and synthesis of the scholarly work on research and innovation and discuss the implications for the future research agenda, particularly with respect to the European Union's framework programs.

For this purpose the project gathered a cross-disciplinary team of more than twenty leading experts from different areas of innovation research, each of who accepted to write a study of a particular aspect of research and innovation. The team included historians, economists, geographers, psychologists and sociologists. Some also had an engineering background. The participants met at two workshops in 2002-2003 to discuss drafts of the various contributions. In addition, a committee composed of Professors Jan Fagerberg (Oslo), David Mowery (Berkeley) and Richard Nelson (Columbia), was at work, synthesizing on the discussions during the workshops and providing advice on how to improve the individual contributions.

The project produced 22 individual working papers, one of which was of a summary/introductory nature. The remaining 21 papers were divided into four major thematic sections, focusing on

- The making of research and innovation,
- The systemic nature of research and innovation,
- Why and how research and innovation differs,
- Research, innovation and performance (including policy).

These papers are in the process of being published as the Oxford Handbook of Innovation (Oxford University Press, September 2004). In addition, for each of the above four themes, a summary report was prepared discussing the state of the art of the area and the relationship to the priorities of the EU Framework Programmes. More information may be obtained from the projects website:
http://tikpc51.uio.no/teari/teari.htm

An emerging agenda: From the "linear model" to innovation systems and beyond …

During the last two decades innovation has increasingly become a central focus for policy makers. The reason for this is the central role innovation is assumed to play for income and employment growth (and quality of life more generally). It is increasingly recognized that high quality science and R&D is not sufficient for the realization of important social objectives. New ideas, important as they may be (with potentially far-reaching consequences), have little economic and social impact unless carried out into practice. This – carrying new ideas out into practice – is what innovation is about, and that is why it so important.

For a long time this seemingly innocent step – carrying new ideas out into practice – was not seen as very significant. The major focus, among policy makers and academics, was on the process prior to the first attempt of commercialisation of a new idea, e.g., science and research (within large public and private sector organizations). As long as investments in science and R&D were kept at a high level, it was assumed that the derived social and economic benefits would follow. This perspective on innovation - which later became known as "the linear model " (Kline and Rosenberg 1986) - has typically been used to legitimate large public investments in science and R&D. It continues to be an influential view, particularly among policy makers. For instance, this type of reasoning concurs well with the recently announced EU policy of raising its expenditure of R&D to the 3% of GDP level.

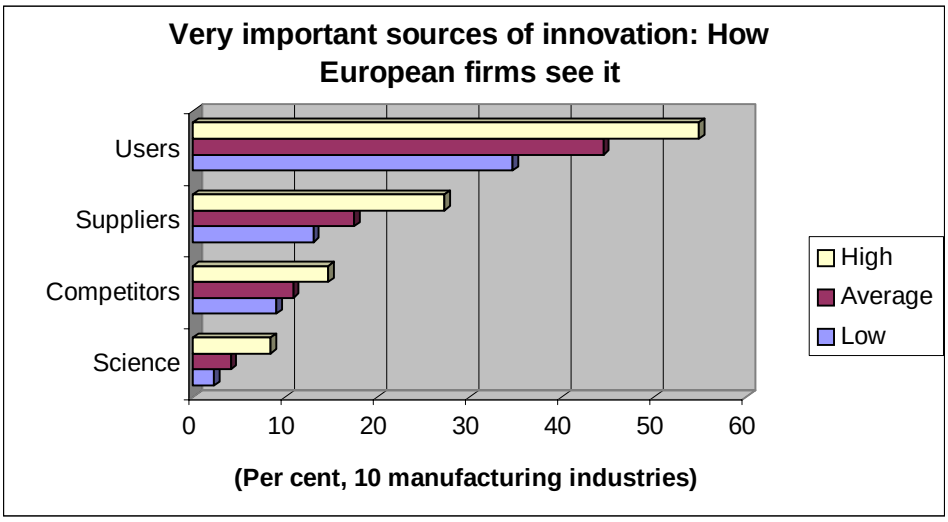
However, although few would deny that science and R&D play important roles in long run economic social and change, the exact nature of these relationships has been subject to considerable controversy. Partly this had to do with the problems in identifying empirically the links between investments in science and R&D and the assumed economic benefits – what has often been termed the "productivity paradox". The Nobel laureate Robert Solow made this observation famous with his remark that "we see R&D everywhere except in the productivity statistics".

Another source for raising new questions about innovation comes from a diverse body of empirical research on innovation processes in firms. Although some if dates way back, this research has been especially vibrant in recent years, particularly in Europe (the so-called Community Innovation Survey – CIS). Figure 1 reproduces the answers obtained from a representative sample of European firms to the question of what the most important sources of innovation are.[1] As is evident from the graph, from a firm point of view, interaction with users is the most important source of innovation, followed by contacts with suppliers and competitors, while contacts the public R&D infrastructure (universities and research institutes, "science" in the graph) are considered to be of much lesser importance. These assessments were remarkably

[1] Firms that consider different actors in the innovative system as 'very important' sources for innovation, expressed as percentage of the total number of innovative firms. The survey (CIS2) was carried out in 1994-1996 in ten European countries (Austria, Belgium, France, Germany, Italy, Netherlands, Norway Portugal, Spain, UK) and covered ten manufacturing industries. Source: Eurostat New Cronos database.

similar in different industries, as shown by the relatively modest variation across industries ("high" versus "low") in responses to the questions posed. In fact, only in one industry did more than one out of twenty innovative firms consider universities and research institutes to be very important for innovation, and even in this industry (chemicals) did less than one tenth of the innovative firms share this view.

Figure 1.



This is not to say that universities do not have an important role to play in a knowledge based economy, but according to firms the most important impacts are of an indirect nature, such as through the supply of highly educated and skilled personnel. Admittedly, these findings may seem to be at odds with the widespread expectations among university administrators these days of substantial future incomes to universities from direct involvement in innovation activities (through intellectual property rights - IPRs). However, available evidence from the US, which spearheaded this movement, indicate that for most universities that have followed this trajectory, establishing an IPR system has in fact been a pure financial loss (Mowery 2004). Arguably, it seems quite likely that for society as a whole, the negative effects of more restrictive practices of knowledge sharing, that naturally follow from an increased emphasis on profiting from IPRs, may well outweigh any positive effects that this movement might have for the universities or personnel involved.

The finding that innovation depends on knowledge sharing and interaction between different types of actors (of which many are external to the firm) led during the 1990s to the formulation of a new approach ("systems of innovation"), which explicitly attempts to take the systemic (or recurring) character of such patterns of interactions more thoroughly into account. This system approach, in its various versions, has become popular among policy makers and analysts, among other things due to its flexible structure (which means that it can easily be adapted to different settings/issues) and the fact that it offers a handy framework for accumulating knowledge about the links between the public R&D infrastructure, policy initiatives/support schemes and firm behaviour. However, it has been argued that (perhaps because of this flexibility) the approach lacks precision both when it comes

to making statements on causality and with regards to providing advice on many the choices policy makers face (Edquist 2004). Hence, following this view, there is an urgent need to "tighten up" the systems of innovation approach.

A way forward?

Arriving at a more complete understanding of innovation, including its driving forces and its role in economic and social change, is an important but at the same time challenging task. An important assumption underlying the TEARI project has been that the most fruitful trajectory towards a more complete understanding may not be to start out from this or that theory or approach and try to perfect it. Rather the guiding principle behind the project has been for the researcher to get to know different approaches - and sources of evidence - and on this basis engage in further exploration of the subject together with researchers with different backgrounds. The argument behind this way of doing things was that the received theories/approaches on the subject all abstract from certain aspects of reality that may be quite important for the understanding of the subject matter.

What became evident through the discussions in the project is that both the "linear model" and the "system of innovation" approach operate at a relatively aggregate level (national, regional, sectoral et.). However, innovation is something that mostly goes on in firms (although in close interaction with outside sources). This implies that to understand innovation dynamics it is not sufficient to focus on interaction with external partners, the resources available, innovation output etc. It is also necessary to take into account what goes on within firms, such as strategic choices, the extent to which it manages to benefit from new ideas ("openness"), what it does to encourage the development of new ideas etc. These are issues that are important for innovation but which are normally not studied under the heading of, say, "innovation systems" (although it perhaps might), but in "strategy", "management" or "business studies".

Thus, an important lesson from the project is that it is essential to transcend some of the existing disciplinary and organizational boundaries that cut across the field and prevent scholars (focusing to a very large extent on the same phenomenon) to interact in a meaningful way. This may not be so easy as it sounds, because the insularity of existing disciplines/specializations tends to reinforce through time through the creation of specialized theories, concepts, modes of communication and exposition etc. that are not fully (or easily) understandable for outsiders. Therefore, cross-disciplinarity – in this as in other areas – does not necessarily come about all by itself, but requires quite strong incentives to thrive. Thus, to arrive at a more complete understanding of innovation, including policy options, it is essential that arrangements are in place to foster cross-disciplinary research, not only across the social sciences, but also across universities and business schools.

Some key lessons

It is not possible to do justice to all the lessons generated through this project, and the reader is advised to consult the original sources for a more extensive account.

Nevertheless, one key lesson from modern innovation research is that innovation is a broad phenomenon, e.g., there are several different types of innovations and all of

these matter. Not only technological innovations of the product and process type, which are what people often use to focus on, but also organizational innovations are important. In fact, many of the most important innovations throughout history have been of the organizational kind such as, for instance, the new distributions system that accompanied the development of mass production in the US a century ago, or how Toyota and other Japanese companies reorganized the entire value chain in the car industry in the period following the end of the Second World War (Bruland and Mowery 2004, Fagerberg and Godinho 2004, Lam 2004). Although some organizational innovations have followed in the wake of technological breakthroughs, and have been shown to be of critical importance for the commercial exploitation of such advances, organizational innovation may also be an important impetus to growth in its own right (the Japanese experience in the car industry is arguably an example of this).

Second, innovation is pervasive. It is not limited to certain so-called high-tech industries, but flourishes in other industries as well, not to speak of services (von Tunzelmann and Acha 2004, Miles 2004), although the factors that matter for innovation (and consequently the available policy options) may vary somewhat from one sector to another (Malerba 2004). Although some innovations may be spectacular technological breakthroughs, this does not necessarily imply that these always are the economically or socially most important. Indeed, the bulk of innovation in modern societies consist of relatively small improvements, and it is probably a safe bet that the cumulative impact of these is as greater (or greater) than that of the more “radical” or “revolutionary” ones. Moreover, different types of innovations are related. Technological breakthroughs typically open up opportunities for a whole range of new innovations (without which such breakthroughs may in fact be of little economic significance).

Third, innovation is a lengthy process that is intertwined with diffusion and fundamentally shaped by the learning undertaken through this process. Most of what we today consider as innovations, such as, for instance, the car or computer, are in fact the result of long historical processes of gradual improvement, and the incorporation of a long series of innovations.

Hence, what this boils down to is that some of the popular folklore surrounding the innovation phenomenon, focusing for instance on the construction of technologically very demanding devices, based on scientific breakthroughs, occurring in big laboratories with the help of very advanced and expensive equipment etc., may be a bit one-sided. Albeit some innovations are of this sort, many are not, including a lot of those that matter economically. As mentioned previously we now have relatively extensive evidence from several countries, based on surveys of innovation activities of firms (see Smith 2004), that consistently shows that what matters most for successful innovation is not so much the link with basic science, big public laboratories or universities, or IPRs for that sake, but close interaction with users (demand), suppliers and competitors (Granstrand 2004).

These lessons, and supporting evidence from the various studies undertaken in the project, may raise important questions for policy. Arguably, in many cases the policy discourse tends to focus too much on the resources available for innovation, e.g., R&D, rather than innovation as such, which – if anything - should be the prime target

for policy. For instance, a well-known concern among policy makers has been that of too little investments in R&D compared to other countries. But such comparisons tend to forget that these figures may reflect differences in specialization patterns, since R&D intensities differ a lot across industries, and countries for various reasons specialize differently. In particular, there exist a number of small, developed economies characterized by a combination of a highly specialized production and trade structure and similarly highly specialized matching competencies, knowledge bases and R&D infrastructures. It is not obvious that it would be a good idea to regard all this as obsolete, just because it is not high R&D, and instead concentrate all efforts in industries (or parts thereof) with much higher R&D intensities. In fact, as argued by von Tunzelmann and Acha (2004), there may be a lot to gain economically from investing in innovation (including R&D) in industries with more modest R&D requirements. On the other hand, it is perfectly possible that there may be industries (or industrial segments) for which the prospects are far from promising, so that a gradual reorientation would be more than justified. The point we wish to emphasize is that to deal with such issues in a constructive manner, a relatively detailed analysis of a country's innovation system – its strength, weaknesses as well the external challenges with which it will be confronted – would be required.

However, even if one should accept the aggregate R&D intensity of a country relative to that of other countries as a natural policy target (which is not obvious), it does not necessarily follow that public investment in R&D or subsidies to private R&D are the only available – or most appropriate - policy options. It is important to keep in mind that differences in aggregate R&D intensities across developed economies are mostly due to differences in private, not public investments in R&D (which tend to be more equal across the developed world). Private investments in R&D, on the other hand, depend on a number of factors, such as for instance the strategic orientation of management, the costs, the perceived risk, the demand for new, innovative products or services and the extent to which R&D is deemed necessary to be able catering for this demand. Hence, innovation and R&D jointly decided. For example, if demand is failing or risk considered too high (which may in some cases amount to the same thing), innovation projects will be abandoned (or not started), and so will the associated R&D investments. Focusing exclusively on the amount of R&D investment, instead of innovation and the wider set of factors that influences it, is equivalent to focusing on the symptoms of an illness rather than the illness itself and its causes. Arguably, it might be just as relevant to turn the question upside down, and ask what policy makers can do the influence the factors that we do know have a large impact on innovation, rather than focusing exclusively on the resources available for innovation, which may not be the constraining factor.

Although the aim of this project has not been to research the relevant policy issues in depth, one recommendation might be to explore the possibility for intervening on the demand side, which after all is one of the most important factors behind successful innovation. This should not be so difficult, given that politicians actually control around half of GDP in many countries (somewhat less in others but still a big chunk of overall demand). One challenge would be how to transform the big spenders among the public sectors|, such as education, health, communication, energy provision (in some countries) etc., into powerhouses for innovation. More generally, what is needed is a transition to an experimental economy; in which experiments with new solutions/technologies would be the normal state of affairs, not the exception.

Such an economy would no doubt generate more innovation, and higher R&D expenditure in the private sector. This would, however, also necessitate a tolerance among policy-makers and the general public for the failures that inevitably would accompany any transition to a more experimental policy framework.

Lessons from (for?) the framework programmes.

After having dealt with some of the more general lessons from the project, we will now turn to the more specific questions of (1) what the framework programmes of the Euroepan Union have contributed the growth of knowledge in this area and (2) the extent to which the conclusions for this project may have implications for future FPs.

Research has been an issue for the European Union from the very beginning, in fact it was mentioned already in the treaty that established the European Coal and Steel Community (1951). But most of this has had a relatively strong technological focus. However, following the slow-down in the world economy in the mid 1970s, a need for a broader perspective on the role of technology and innovation in long run economic and social change emerged, and this resulted in a series of European Community research programmes focusing on Forecasting and Assessment in Science and Technology (FAST and – later - MONITOR). Research in these programmes focused mainly on particular technologies and/or sectors, such as ICT technologies, biotechnology etc., but also to some extent on more general issues such as globalisation, regionalisation, competitiveness, employment/work etc..

A major expansion in the finance of social science research at the European level came in 1994 with the Target Socio-Economic Research Program (TSER), which was part of the fourth framework program. This program was based on the idea that the fragmentation of the social sciences hindered the development of policy relevant research centred around important social and political objectives. Hence the program had a strong cross-disciplinary appeal. It was targeted towards three specific areas, one of which was “evaluation of science and technology options”. Similar objectives were also central in the fifth framework program from 1998 onwards, which contained a so-called “key action” especially devoted to socio-economic research. One of the thematic priorities within this “key actions” was “technology, society and employment”.

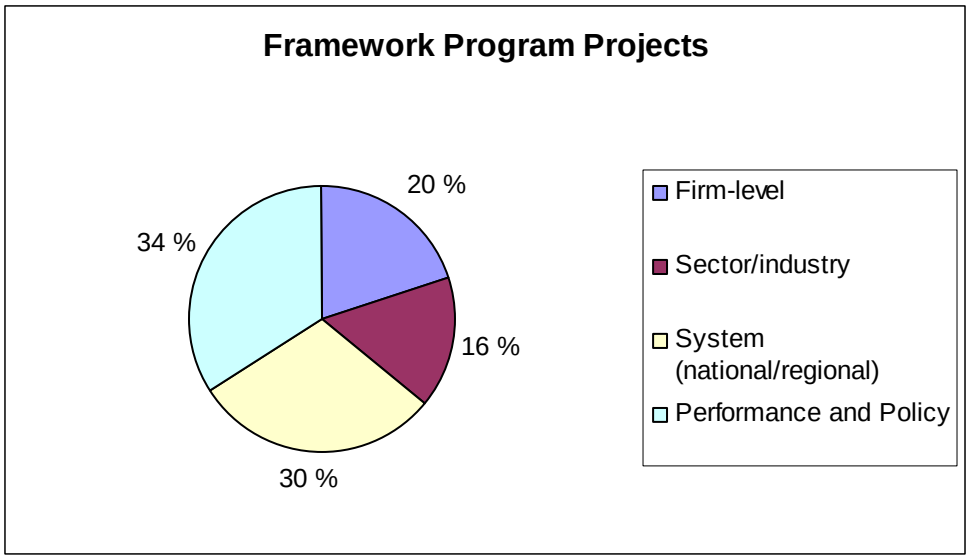
There is no doubt that European programs in this area, and in particular the TSER program, represented an important stimulus to cross-disciplinary research on the role played by science and technology in economic and social change, but the precise extent of this is difficult to assess given that this has to best of our knowledge never been properly researched.[2] However, to throw some light on the issue we decided to explore the extent to which the thematic priorities of the research undertaken in the TSER and key actions in the fourth and fifth framework program concurred with those that follows from the TEARI project.

[2] See, however, Benavot and Gross (2004) for an analysis of the effects on the framework programs on social science more generally and Kastrinos and Guy (2004) for an assessment of the achievements of the TSER program.

The TSER and "key action" projects can be accessed through the web through http://improving-ser.sti.jrc.it/default. At the time the search was conducted the data base contained 276 projects (today this is somewhat larger, more than 300). A preliminary screening of the project titles and summaries indicated that about one third of the projects focused on issues of relevance for "technology, research and innovation". Excluding projects that were not finished by the end of 2002 (and for which only scant information was available) and some projects that by closer scrutiny were found to be irrelevant reduced the number of relevant projects to 70. These 70 projects were classified into four main classes, comparable to the four sections of the TEARI project (firm-level, system-level, sector-level and performance/policy), and a number of sub-categories within these classes.

Figure 2 presents a division of the projects into the four main groups. As is evident from the graph, there is a strong bias towards more "macro-oriented" issues, as about two third of the projects fall within the "performance and policy" and "system" camps. Looking in more detail at the distribution within these two broad classes, many projects within "performance and policy" focus on employment issues or specific methods, techniques or data of relevance for policy making. In contrast there are relatively few projects on the effects of research and innovation on growth/productivity, and hardly any that focuses on competitiveness (though there are some larger projects that integrate these issues within a broader framework). Many of the "system" projects focus on specific parts of the system, such as R&D and education, or regional issues, while there are - surprisingly perhaps – very few projects that focus on the national systems of innovation as a whole.

Figure 2.



About one third of these projects focus on the firm or sector level. The relatively few projects (about one fifth of the total) that were classified as belonging to the firm level, may be further divided into three sub-groups; training, networking and management/work organisation. What is most striking is that there was hardly any project that focuses on innovation processes in firms. Given the importance of

innovation for economic and social change, and the role of firms for innovation, this must be seen as a glaring omission. The sectoral projects spread over a large number of sectors/industries, from chemicals and biotechnology via telecommunication and software to services. Most of it focuses on what may be labelled “high tech” (or is commonly classified as such) while there is not much attention on the “low-tech” sectors, which, as pointed out by von Tunzelman and Acha (2004), are very important economically.

In conclusion, the survey undertaken confirms that the European Union has been an important source of funding for social science research on the role of research and innovation in social and economic change. Benavot (2004), in an analysis of the impact of the framework programmes on European social science, found that these programmes had contributed to a strengthening of European social science research, a clearer European identity among the participating researchers and more widespread cross-disciplinarity. There is no reason to believe that these effects do not hold for the more narrowly defined subset of social science research studied here. However, the research supported by the European Union in this area has been shown to have a strong bias towards “macro-oriented” and “sectoral” issues, some of which have been priorities in relevant EU research programs for a long time. It seems appropriate to point out that the priorities that are revealed in this way do not really reflect some of the most the pressing issues as identified through the present project in an adequate manner. If innovation is the source of long run economic social change, and EU wishes to reach its objectives (Lisbon Summit, March 2000) of being “the most competitive and dynamic knowledge-based economy in the world”, a much stronger socio-economic knowledge base needs to be created to support the process leading to that aim. This can only be achieved by creating a cross-disciplinary community of scholars devoted to innovation research, and in that respect the basic philosophy underlying the TSER program and later initiatives was right. However, a stronger targeting towards central issues that have been neglected for too long may be required, and an ability to identify (and stick to) critical issues on which more knowledge may be needed, even if a strong research community (disciplinary or otherwise) lobbying for it may be lacking.

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